

UNIVERSITY OF LONDON



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DUCANE ROAD
LONDON, W.12
23rd July, 1952.

Dear Cavalli,

Many thanks for your letter of the 4th July and your later air-letter. I am sorry to have troubled you about the question of money and greatly appreciate your offer to help should we run out, but I am sure that this will not occur. We had heard that Italy was an extremely expensive place to have a holiday and we were both worried as to whether twelve days was too long a stay for our resources.

I hope you will not abandon the idea of giving a paper yourself at the Pallanza meeting, since I personally would very much like to hear it. I also think that since what I have to say will be biased in favour of my own views, and I hope provocative, that the opposing interpretation should be given equal prominence.

I have not yet received your paper for the J.G.M. but I will be only too pleased to forward it on arrival and to look after the proofs when they come through. I did not like to press this suggestion too much in my last letter since I thought you might prefer to have someone else, like Pontecorvo, do it for you, but now that you have suggested it yourself I will be very pleased to do so.

As regards filtration experiments, all the collodian filtrates of 0.74 μ . A.P.D. that I have obtained have been sterile. I mentioned in the copy to you of the last letter I sent to Lederberg that I had appeared to get a few F- \rightarrow F+ conversions with one of these filtrates. On checking up on these conversions however I

/find that they were not

found that they were not reproducible using my standard recombination technique. It seems therefore that the F+ agent was highly unstable in the converted strains, *if, in fact, they had been converted at all.*

As regards your remarks about prototroph yields with SM-sterilised F+ cultures, my yields have always been appreciably and sometimes greatly lower than with untreated cultures. Perhaps I did not stress this sufficiently in my communication to Nature. My main point was that whereas I have never obtained a completely infertile SM-sterilised F+ strain, SM-sterilised F- strains have been invariably infertile. I have rather got away from these initial experiments (though I will have to get back to them soon) but it seems to me that the fall in productivity of SM-treated F+ suspensions could be accounted for by one or both of two factors. The first is that since streptomycin has a high affinity for nucleic acids it may ~~also~~ ^{also} alter the physical state of the transmitted chromosome moiety ~~when~~ ^{so} only a proportion are capable of participating in recombination. Alternatively when washed F+ and F- suspensions are spread together on minimal agar the F+ strain is in a physiologically and metabolically dynamic state so that, according to my view, "gametes" can be progressively liberated over a considerable period of time. On the other hand, treatment with streptomycin will "freeze" the F+ culture so that it is no longer physiologically dynamic and only those "gametes" which had been liberated at the time of treatment will be active.

I have only done a few experiments with other agents, including heat at 60° for thirty minutes, but found that they were ineffective in differentiating F+ and F- cells. This is another aspect of recombination which I hope to get back to.

I hope you enjoy your holiday and your trip to Paris. I have just returned from Dublin and had a very hectic time visiting many friends and relations.

Looking forward to seeing you at Pallanza, . . .

Yours sincerely,

William Hayes

will send tomorrow

P.S. I ~~enclose~~ a copy of my own paper to the J.G.M.

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